

assert in this respect, it is clear that in the case of the *Captain*, they thought this amount of injury possible, and it is equally clear from the quotations given by Mr. Goschen in Parliament, that they thought the same of the *Inflexible*, when they proposed that she should be built, and thought this, notwithstanding the introduction of certain cork-filled chambers and other sub-divisions upon which they now seem disposed to rely for the ship's safety. We may add that even during the present controversy, Mr. Barnaby has published figures which assume the total annihilation of the ends, and if they can be totally annihilated it is clear they may be so far injured as to lose all buoyancy and stability. We may confidently assume, therefore, that the ends can be so far wounded and damaged as to cease to help the ship's stability, and therefore to leave her wholly dependent upon the citadel for the power of keeping from capsizing. In Fig. 3 we have shown several large injuries, such as we may assume modern shells are fully capable of inflicting, merely to help the reader to get clear ideas on the subject.

The question now at issue really is, therefore, what amount of stability has the ship (by virtue of the citadel) with the ends thus injured? The *Times* and Mr. Reed say that careful calculations which have been made show that she has none, or next to none. Hitherto the Admiralty have refrained from saying how much they claim for her. They say that the *Times* and Mr. Reed are entirely wrong in their calculations, and that the ship really has abundant stability for all purposes of safety, and they appeal to a model which is at the Admiralty to prove this. Let us say at once no model can possibly prove anything of the kind; the model must be weighted and arranged entirely to represent the results of calculations, and it is these results which should be clearly, and fully, and authoritatively stated. The Government have laid certain papers on the table of the House of Commons, but they are not yet published, and until they are in our hands it is impossible to pursue the subject further. We shall hereafter give due consideration to them. All that we can now say is that with the *Captain* case fresh in our memory, in which the Admiralty office dangerously

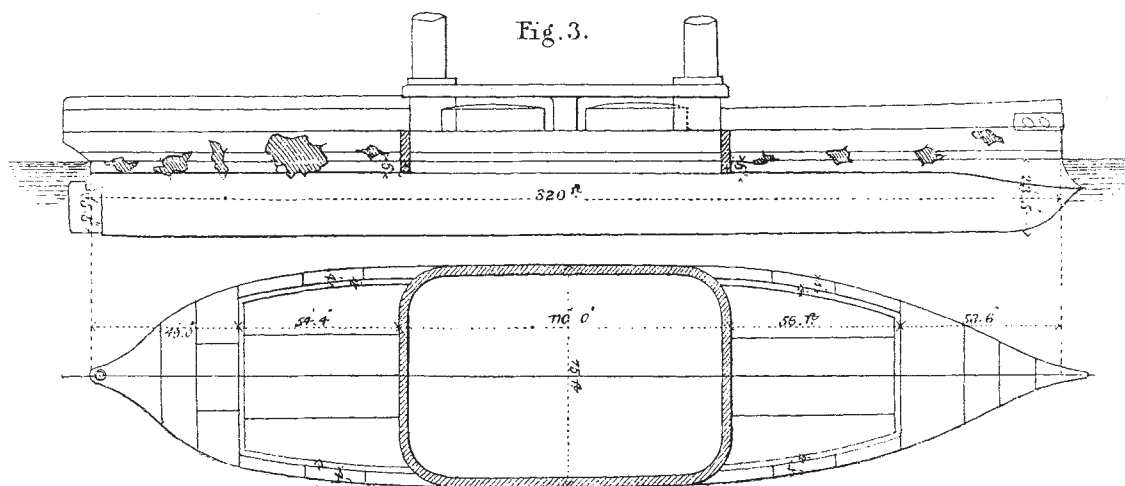


Fig. 4.

overrated the safety of the ship in this very respect, and, remembering as we do that for a ship to be safe at sea, she should have a very large margin of stability over and above that which mere statical and smooth water conditions point to, we shall not ourselves be satisfied with less than the Committee on Designs laid down, viz., "that the angle of vanishing stability should not be fixed at less than 50 deg." Nor shall we be content with this if this range is obtained only in conjunction with a small amount of stability from point to point. Mr. Reed has pointed out, in his letters to the *Times*, the great danger of considering range only, and has attacked the dictum of the Committee on this ground. Dr. Woolley, one of its scientific members, has replied, admitting the accuracy of Mr. Reed's view, but explaining that the truth he enunciates is so elementary and obvious that the Committee thought it unnecessary to mention it, and would indeed have considered it "impertinent" (in the proper sense of the word) to state it. It is difficult to take this view of the matter, however, when we remember that the highest scientific officer of the Admiralty, in a matter affecting the

safety of four of H.M. ships of the largest and newest type, has seized this dictum of the Committee as a sufficient and satisfactory guarantee of their security. We fear we must conclude that the Committee either neglected a very serious element in the calculations, or else greatly overrated the skill and discernment with which their words would be interpreted.

THE DEVELOPMENT OF THE OVUM¹

Bütschli on the Earliest Developmental Processes of the Ovum, and on the Conjugation of Infusoria.

Studien über die ersten Entwicklungsvorgänge der Eizelle, die Zelltheilung und die Conjugation der Infusorien. Von O. Bütschli. (Frankfurt, 1876.)

II.

COMING now to the large and important question of the *Conjugation of Infusoria*, its nature and bearing upon the life-history of the forms, we are bound to state at once our conviction of the inefficiency of the observa-

Continued from p. 180.

tions recorded on account of their discontinuity. Nothing but a close and continuous observation of the same forms extending over an entire life cycle, repeated again and again, can lead to absolute results. Errors fatal to the interests of truth inevitably arise, when minute organic forms are studied, not by continuous watching, but from inferences made from the phenomena manifest at different periods, the intervals between which are blank. Further, whilst the use of reagents on the dead forms taken at various stages is of the utmost value, when they are examined side by side with continuous observation on the living form, these may be not only not instructive but misleading when taken by themselves.

Bütschli's observations are numerous and interesting, but their value will be best estimated, by understanding briefly the nature of the hypothesis they are declared by their author to indicate. Put in its shortest form, it is that conjugation amongst the Infusoria is simply a *rejuvenescence* of the creatures which undergo it, enabling them to become "the stem ancestors of a series of generations" which propagate by fission. As yet the process of rejuvenescence has had, in biology, a limited application, being noticed in the formation of the swarm-spores of *Cedogonium* and other of the lowliest plants; but its connection with sexual reproduction is not clear, as no union of different elements has been made out, and it is by no means certain that the whole process of reproduction is exhausted by it. When, however, it is combined with conjugation, as in the Bacillariaceæ, it becomes plainer; although, so far as is known at present, it by no means follows that the whole generative process in these forms is known; but it is to the *Auxospores* by which rejuvenescence is secured in these forms that Bütschli appeals for the support of his theory of infusorial conjugation. Pfitzer and Schmitz have made what are at present the most complete observations of the phenomena in question; from which we learn that the customary mode of reproduction is by fission, but at each repetition the individuals dwindle in size, until they can apparently go no farther,¹ then the conjugation of two individuals takes place, the formation of auxospores being the result, that is to say rejuvenated individuals; and from these a new departure of fissiparous generations takes place, well observed by Schmitz in the case of *coconema cistula*. There is no coalescence; the frustules simply lay themselves parallel to one another, they become surrounded by a common envelope of mucus; the protoplasm of the cells comes into contact, each frustule grows larger and becomes an auxospore. What the influence is which these frustules exert upon each other is wholly unknown; but that it has a real existence is shown in the result; each auxospore forming a stem ancestor of a new series.

This is what Bütschli extends to the infusoria, and contrary to the interpretations of Balbiani, Stein, and others, maintains that the act of conjugation so well known amongst the Paramecia, Vorticellæ, &c., is not a precursor of sexual products, but simply a means by

which these forms, exhausted by continued fission, become more highly vitalised and rejuvenated, and again enter upon the process of fissiparous multiplication, which indeed becomes thus their only method of increase.

It should be noted that on the whole the facts adduced by Balbiani and Stein are admitted, but they are submitted to a wholly different interpretation; and it is specially insisted on that the forms that go into the conjugation state are of a minimum size; which fact Balbiani explains as the result of a special development for sexual purposes, but this is disallowed by Bütschli, who insists that it results from exhaustion of vitality at the terminus of a series of fissiparous multiplications. Indeed these weakened and minimised forms unite in conjugation and are neither absorbed into each other nor produce embryos, but increase in size and vitality, separate, and commence again the fission by which alone increase is effected.

The truth of this is insisted on as deriving strong support from some of the very remarkable external changes which the author has seen certain of the Infusoria undergo. In *Euplotes* and *Oxytrichineæ* a great part of the *ciliary system* is said to perish towards the end of conjugation; and afterwards, when separation takes place, to be again renewed. In *Colpidium colpoda* the entire mouth was lost in conjugation, but was renewed again after separation. So in *Bursaria truncatella*, the conjugated animals, it is affirmed, lose entirely the complex apparatus of the peristoma, which by a new growth after conjugation is restored. So also there is declared to be a complete rejuvenescence of the more important internal parts. The "secondary nucleus" in *Stylonichia mytilus*, and in *Blepharisma lateritia* and *Colpidium colpoda* the old nucleus is said to be eliminated and a new one formed. In others, part of the nucleus is thrown off, and part renewed; in others a new nucleus formed and coalesced with the old one. From these and similar observations it is inferred that the "essence of conjugation consists in the rejuvenescence of both the individuals;" and that this is chiefly centred in the "secondary nucleus" which is declared to be of the utmost importance in the life of the creature.

During the process of conjugation, also, the plasma-contents of the individuals have been seen to interchange; this especially in *Oxytrichineæ*, but also in other infusoria.

Against Balbiani's hypothesis—that the nucleus is the ovarium and the nucleolus the testis, containing spermatogenic elements—Bütschli affirms that in *P. aurelia* and *P. colpoda* the supposed spermatogenic capsule in some cases wholly disappeared without any following change in the nucleus that could be discovered, and that consequently it did not effect fertilisation. In short, he believes that the observations he has made are quite competent to overturn the sexual hypothesis in these organisms, and to establish that of *rejuvenescence* in its place.

That there is extreme ingenuity in this hypothesis we readily admit; that there is also the utmost conflict of interpretation amongst the best observers of these organisms, we admit with equal readiness. But that the author's observations give *scientific* sanction to his theory on the one hand, or either explain away or harmonise the labours of his predecessors or *collaborateurs* on the other, we are fain to dispute. The exhaustive and continuous

¹ It is impossible not to notice here the extremely interesting and certainly somewhat remarkable paper of Dr. Wallich in the February number of the *Monthly Microscopical Journal* for 1877, "On the Relation between the Development, Reproduction, and Markings of the Diatomaceæ;" for in this paper what is apparently the *auxospore* of Pfitzer and Schmitz is called the *sporangial frustule*. But this, instead of having dwindled in size before conjugation appears to have become enormous in proportion, and within this the "new parents of the race arise," and from the conjugation of these the new forms spring as daughter frustules.

method of observation—following a single form through all the phases of its life—has never been thoroughly adopted; and conflict of interpretation inevitably arises. Bütschli has fallen into the same groove, and his results, although valuable and full of suggestion, have no irresistible meaning. They present points of new departure for hypothesis, and nothing more.

Nor can we be quite certain, from the evidence afforded, of the correctness of the larger and more important of the facts stated. We want, for example, more than a mere statement that the "ciliary apparatus" and the important organs of the *peristoma* were actually destroyed by conjugation. That they are suppressed—flattened—deranged by prolonged contact, we have observed again and again in several forms, especially *Stylonichia*, *Pustulata*, and *Mytilus*; but they rapidly regained their *normal condition*, and certainly did not *grow afresh* by "rejuvenescence" as in the cases stated by our author. And this is certainly of moment. In some important sense also this will apply to the nucleus and nucleolus themselves. Doubtless the investigations of Bütschli on the metamorphoses of these bodies, especially the latter, in such forms as *P. bursaria*, *aurelia*, *putrinum*, and others have a large importance; and if they should be confirmed by *continuous observation on the living form*, controlled by the evidence of preparations, made at short intervals, under the influence of acetic and osmic acids, and other reagents, not only will Balbiani's hypothesis become modified, but a sequence will be given to the successive stages, often now wanting, in the observations of Bütschli himself. It is impossible not to be struck, for example, with the minuteness of his observations, made on the nucleolus changes in *P. bursaria*; but they are utterly incompetent to accomplish his own purpose and establish his own idea. He declares that both Balbiani and Stein utterly mistook the destiny of the nucleus and nucleolus; and quite repudiates the changes said to come upon the nucleus during conjugation. But to establish his own hypothesis the whole process of morphological change in the nucleus at least should have been followed, and not once but many times. Yet the very first complete change effected in this organ could not be explained; and after following it into fission as the result of conjugation, he observed four "nucleolus capsules" as the issue, in each paramæcium. Two of these became light and clear; the other two diminished in size, and became fibrous, but on the second day they lost their fibres and became homogeneous and dark; and on the third day—*vanished!* that is to say, by the method pursued by the observer, they were lost, and "no trace of them was to be found." From this Bütschli concludes that they were "cast out," and no further concern in relation to them is evinced! Yet it must be remembered that Balbiani describes a similar condition of the same forms, and considers the granules germs or ova. To deal thus lightly with the ejection of apparently organised bodies in a set of observations designed to *prove* that what have been considered ovarian, or at least sexual, products, was erroneous, is certainly remarkable. Clearly no result can be arrived at until the manner of the vanishing of these bodies be understood; and if they were ejected, until their future destiny became known. This is all the more imperative from the fact that *after* the ejection of the "bodies," the paramæcium resumes its

normal condition in size and appearance, although the method by which this conclusion is reached is by saltative inferences, and not by continuous proofs.

Again,—in *B. bursaria* and *aurelia*, two "light bodies"—definite products of the nucleolus—are repeatedly seen in successive stages after conjugation, but having been followed to a certain point we are told that "the further destiny of these two light bodies escaped me!" and yet it is assumed that the life history of the creatures is known.

Again,—in these same forms the *nucleus* broke up into a hundred spherules; and yet our author frankly declares "I am not quite certain of the destiny of the . . . fragments of the old nucleus!" This is the more important since Schaafhausen affirms that he has seen *P. aurelia* lay or deposit ova; "the organisms crammed full of egg-spheres, surrounded with clear fluid, extrudes in an hour several times one such egg."

Again,—in *Colpidium colpoda*, after conjugation, two small light spheres appear, these the author "thinks most probably" grow out of the nucleus capsules, while the *nucleus* itself is *cast out*; Bütschli followed it "for some time" and then it was lost, so he does not know its final destiny! Of what service can all the subsequent transformations of the organism itself be when this ejected organism is assumed to mean nothing? In *Blepharisma laterita* a number of "nucleolus-like bodies" were found by "squeezing and acetic acid," but their destiny was never found; while on the third day after conjugation "the *nucleus* which had been present up to this time was not to be found," and so the author meets the emergency by supposing that it was "cast out," and of course had no meaning in the history of the organism. So also in *Chilodon cuculus*, we are told that the "destiny of the original nucleus remains undetermined." In the conjugation phenomena of *Stylonichi mytilus* there is an equal or even more grave defect.

In precisely the same way in the attempt made by Bütschli to establish the position he occupies that the embryonal regions of Balbiani and others as existing in these lowly forms are to be entirely explained by the presence of swarm spores of internal parasites, there is the same want of perfect sequence, and the unscientific "no doubt" which is made to supply the place of facts.

But our space is exhausted. We have not referred to the above defects with any attempt to depreciate a valuable book. It is because it is strong enough in important facts to be a help in the unravelling of biological difficulties that we have not hesitated to point out the difference between the *theories* and the *facts* which it contains. To have attempted exhaustive criticism of such a work would have involved four or five times the space occupied by this article; but after a careful perusal and reperusal of its contents, we are obliged to admit the ingenuity of the author both in the work he has done and the method he has employed for interpreting it. But it is to the former that we attach by far the most importance; for whilst there are many missing links in evidence which make conclusions from the whole unwise, there are facts given us which must help future observers and land us nearer to the desired truth.

It may be finally observed—*i.* That if the theory of rejuvenescence, as put and insisted on by Bütschli, be established for any one form, conjugation should have no

other meaning or place in any part of its history than rejuvenescence can explain. Now *Stylonichia pustulata* is amongst the forms the author has seen to conjugate, and as he believes, as a consequence, to become simply more vital and larger for renewed fission. But Engelmann is undoubtedly right in his affirmation, that there is a conjugate state in which these organisms do not again separate, but the pair simply fuse together. One of the writers of this paper has observed it repeatedly under conditions which render error impossible; this is not the place to consider to what this fusion leads, but it is important as a fact, inasmuch as it throws doubt upon the completeness of the theory of rejuvenescence, even supposing the facts given us by Bütschli led without exception up to it. Bütschli even admits that this process of fusion may happen, but he simply dismisses it as a "very unusual one"—surely all the more important on this account, inasmuch as we know that in more highly organised creatures not only a long time, but generations may intervene between distinct acts of fertilisation.

2. It does not follow that if rejuvenescence be rejected to the extent and with the meaning Bütschli gives it, that it must be rejected altogether. He gives us many remarkable facts that deserve further experiment and research; and it may result, that what he calls rejuvenescence, is one of the many modes by which rapidity of fissiparous multiplication is in some organisms aided, and the necessity for the true act of fertilisation is made less frequent; and

3. It is clear that there are points in the theory of Balbiani which the facts given by Bütschli overturn; while there are others that certainly remain unshaken, if they be not strengthened. But it is needful to remember that if the facts given by Bütschli wholly invalidated the interpretations of Balbiani the theory advanced by Bütschli by no means follows as a consequence. In the present state of this inquiry we must seek facts industriously, and with persistent honesty, and be assured that their accumulation will lead to important issues; but we shall do well to place theory, however fascinating, in an extremely subordinate place.

W. H. DALLINGER
J. DRYSDALE

VON RICHTHOFEN'S "CHINA"

China. Ergebnisse eigener Reisen, und darauf gegründeter Studien, Von Ferdinand Freiherrn von Richthofen. Band I. (Berlin: D. Reimer, 1877.)

WE are glad to welcome the appearance of the first volume of this long-promised work from the pen of the well-known geologist and geographer, Baron v. Richthofen. We content ourselves at present with a general account of the work, hoping in an early number to be able to examine it in detail. The author has enjoyed rare facilities for the accumulation of material, and has improved them so thoroughly that the published results of his researches will assume a leading position among the late additions to scientific literature. In 1860 he accompanied Count Eulenberg on his mission to China and Japan for the purpose of closing commercial treaties between these lands and the German states. On the return of the expedition Baron v. Richthofen lingered

behind, attracted by the many unsolved problems of the Celestial Empire. Up to 1872 he devoted himself to a systematic, thorough investigation of the geography and geology of China, traversing in the course of seven different journeys the whole eastern part of the empire from Canton to Corea, and penetrating westward to the sources of the Yang-tze-Kiang and the frontiers of Thibet. The essential aims of the traveller were to place on a scientific basis the geography of the land, determining the hypsometric relations, and the laws governing the conformation of the mountain-chains, to examine the general geological structure, especially in its relations to the great basins of Central Asia, and to study the laws of climatic changes. Other scientific questions received a minor consideration, and the intellectual life of the people was left entirely out of view. The present volume forms little more than an introduction to the elaboration of the immense number of observations made during the long series of years, which will form the body of the work. It is mainly occupied with an extensive and complete description of the growth of our knowledge with regard to China, forming a valuable index to the literature on this country. No small amount of space is devoted to the book, "Yü-Kung," or imperial geography, forming the sixth in the series of historical works attributed to Confucius, and covering the period 2357-720 B.C. The remaining portion of the volume is occupied with the geographical relations of China to Central Asia, and contains a most important study of the loess regions of Northern China. They are not only considered in their relations to the saline steppes of Central Asia, but are compared with all the great loess formations known, and supply the basis for an interesting theory with regard to the formation in the one case of fertile valleys, as those of the Nile and Mississippi, and in the other of sandy wastes. Scarcely less valuable is the clear and distinct picture afforded of the whole mountain system of this portion of Asia. The author finds the laws governing the conformations so simple, that less time was required to determine the system than would have been necessary for a tenth of the area in Europe. In a closing chapter on the problems of modern scientific geography, the author sharply defines the province of his science, drawing clearly the limits between it and political geography, ethnography, and kindred sciences. The method to be used in the solution of these problems he defines as "the uninterrupted consideration of the causal, mutual relations between the earth's surface from its various points of view, terrestrial physics, and the atmosphere on the one side, and between these elements and the organic world in its broadest sense on the other side." Of the three volumes yet to appear, one will be devoted to palæontology, in which the author will be assisted by Dr. Kayser, Dr. Schwager, Prof. Schenk, and other able geologists. The remaining two will contain the author's extended researches into the coal-fields of China, regarded by him as more valuable than the deposits in the United States of America—the geological structure of the land, the climatic phenomena, the population as affected by these two agencies, the river system, means of transport by land and water, chief productions, mercantile possibilities, &c. A generous grant from the Emperor of Germany has permitted the publication of the work in a most sumptuous